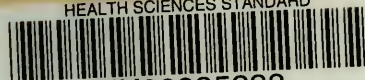


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# CHLOROFORM SYNCOPE.

ROBERT KIRK, M.D.

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A NEW THEORY

OF

# CHLOROFORM SYNCOPE,

*SHOWING HOW THE ANÆSTHETIC OUGHT TO BE  
ADMINISTERED.*

BY

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## PREFACE.

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NOTHING but a strong conviction of being in the right could justify the author of the following pages for expressing views which must give a violent wrench to pre-conceived opinions, and which will almost certainly be met with opposition from all parties. These views, however, it may be stated, are not a thing of yesterday, having been present to the mind of the writer, in a more or less definite form, for fifteen years before they finally crystallised in their present shape. During that period new facts have been added to our knowledge, which, together with the complete failure of all attempts to look for an explanation of chloroform syncope in other directions, have only tended to confirm his belief in the soundness of his opinions. Various reasons might be assigned why the theory here advanced has not been published earlier, and some of these, but not all, may here be stated. In the first place experiment, although it gave some countenance to the author's view, did not fully prove it, as will be seen in the sequel. Next just as he had concluded his investigation of the question, there appeared the first report of the Glasgow Chloroform



A NEW THEORY  
OF  
CHLOROFORM SYNCOPÉ.

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**Introduction.**—The most interesting question in anæsthesia centres in the causation of those lamentable, and apparently anomalous and inexplicable fatalities which have occurred with chloroform in the human subject. Two theories, diametrically opposed to each other, have been advanced as to the cause of these accidents; one being that they were due to an overdose of the poison having been used, and the other that they have in some way or other happened when no overdose, but rather an underdose, had been administered. The rules laid down for the administration of the anæsthetic have varied accordingly. The first representatives of the one school were Sir James Simpson and his colleague, Prof. Syme. "We give the patient good chloroform, and plenty of it, and we never have accidents here," was the oft-repeated utterance of the latter, while the language of Simpson was

still more forcible. "Put the patient as quickly as possible under a strong and overwhelming dose of the poison" was one of his injunctions, which, slightly varied, will be found to recur frequently in his works. Both insisted not only that the patient should be deeply anæsthetised, but that deep narcosis should be kept up throughout the entire operation to be performed, and they attributed fatal accidents to the interminable struggling which supervened when the chloroform was too slowly administered, or to the shock of the operation, which, in their view, might have been prevented if the narcotism had been complete. In their views and practice, however, they have been followed by no other school, and let us now contrast the above with some of the statements of the exponents of a different doctrine. The following from Dechambre's dictionary\* reads like a direct reply to Simpson's instructions: — "Quelques chirurgiens ont conseillé de donner d'emblée et dans le plus court délai possible, une grande quantité de chloroforme afin de traverser rapidement on de supprimer la période d'excitation; on sidère le malade, comme on dit. C'est une méthode des plus dangereuses, non seulement parce qu'elle expose à dépasser le but, à produire brusquement un état de collapsus inquiétant, mais surtout parce qu'elle provoque ces perturbations contingentes du côté du cœur,

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\* Art. Anesthésie Chirurgicale.

qu'il importe surtout d'éviter. Nous ne saurions trop recommander de procéder aux inhalations par gradations insensibles, en commençant de telle sorte que l'impression locale des vapeurs soit à peine ressentie."

This latter method of administration has been approved of, in general terms, by Claude Bernard, the London Chloroform Committee of 1864, by Dr. Snow, and numerous others. It has not only the advantage over the other of being apparently more reasonable, but has also been supported by the evidence derived from numerous careful and laborious experiments on animals. To the present day, however, the controversy remains in the same position as when it was first started. When Sir George Macleod, in a clinical lecture,\* expressed the opinion that he feared more deaths were due to too little than to too much chloroform being given, and that drops might kill when drachms would save; and when this was immediately condemned by the late Mr. Clover as an erroneous and dangerous doctrine, we had a comparatively recent illustration of the same irreconcilable difference of opinion amongst authorities on this subject.

These contradictions appear extraordinary, but I think it may be satisfactorily shown that Simpson and Syme did not employ an atmosphere of a too concentrated kind, one which could be proved by experiments on animals to

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\* Brit. Med. Journ., Jany. 1st, 1876, p. 7.

be dangerously strong. The most important outcome of the different teaching of the two schools was probably this, that the followers of the one went boldly on with the administration without intermission until true surgical anæsthesia was complete, while those of the other interrupted the inhalation frequently for various symptoms, indicative of real or imaginary danger, and stopped short at a stage of anæsthesia thought just sufficient to enable the necessary operation to be performed. It is generally admitted that the former is entitled to claim the greatest amount of success. Of ten thousand cases in the practice of Simpson and Syme, only one proved fatal, and hardly any will doubt their conscientiousness and veracity in this statement of results. The solitary fatal case occurred in the hands of the former, and was immediately published. Almost unbounded confidence in the safety of the agent is still entertained by surgeons in Scotland, and we often have expressions of a similar trust in the medical journals from various practitioners, some of whom confess to using it in a free and apparently reckless manner. The latest of these has come from Surgeon-Major Lawrie, and is as emphatically and strongly expressed as any that have gone before. It would appear that there must be something in connection with the subject which has never yet been satisfactorily explained, and it remains to be seen whether any light is to be derived from the new theory which I here venture to propound.

**The New Theory.**—The theory of chloroform syncope here advanced is one which I have long entertained, and of the truth of which I have become more strongly convinced as time has passed, and further facts have accumulated, but it differs so radically from current views that I might still hesitate to publish it, for no doubt it will be received with incredulity. In my view of it, the solution of the problem has proved impossible because it has been sought for where it was not to be found, namely, in the sole action of the chloroform in the blood. Were this the only effect, the agent might be regarded as a single force acting in one direction, and the effects should have been uniform and easily understood. Experience has proved, however, that the phenomena are too complicated to be explained in this way, and that there is still something wanting. The new theory takes its starting-point from the hypothesis that there must be an effect produced by the vapour on the pulmonary mucous membrane, and that we have this also to reckon with. The idea of such an action of an anæsthetic vapour is not novel. Lallemand, Perrin, et Duroi, in their work *Du Rôle de l'Alcool et des Anesthésiques dans l'Organisme*, p. 253, expressly assume that such vapours have a direct paralysing influence on the extremities of the vagi, that they diminish the oxygenation of the blood, and suspend the *besoin de respirer*,—a true observation, as it appears to me, but one of which they did not see



the full importance in the case of chloroform. Dr. Snow does not notice this subject, and neither the London Chloroform Committee of 1864, nor Claude Bernard, admit any action of the kind, except an irritating and asphyxiating effect from concentrated atmospheres. The latter must have been wrong, however, if it should be found that the supposed anomalies of chloroform can be explained by the hypothesis that the vapour produces an action and reaction of sufficient intensity here.

It is at once evident that if, besides the action of the chloroform dissolved in the blood, there is a further effect due to the vapour in the pulmonary cells, one half of the field of inquiry has hitherto lain hidden from view, and no wonder the action of the anæsthetic has remained a mystery. If there be this action on the pulmonary mucous membrane the agent must be regarded as two forces, acting at two several points, one without and the other within the circulation, and the resulting phenomena are to be ascribed to the action and reaction of these two forces, and to the varying degrees of intensity with which they operate at different stages of the inhalation. In considering these forces it must be evident that the first, the external one, will soon reach its maximum of intensity, and at a period when that of the second is so slight that for the sake of simplicity it may be left out of account. We have next to consider the probable reaction of such a force, that is, the effect likely



to ensue when it is withdrawn and ceases to act. Clearly, this is determined with mathematical exactitude by a definite principle—the effect will be in precise proportion to the intensity with which the force was acting, and to the suddenness with which it is withdrawn. Now the action of the force must be the ordinary anæsthetic effect of the agent, causing retardation and stagnation of the corpuscles in the vessels, and a diminution of all the processes going on there, attended, through reflex action, with corresponding results on the systemic circulation. If we suppose that these processes are diminished by a fourth, then the effect may be compared to that of a tourniquet compressing one of the iliac arteries; if by a half, to a similar compression of the abdominal aorta. It must be evident that if such be the intensity of the force, its sudden withdrawal will be attended with the same liability to syncope as would be the sudden removal of the tourniquet in the two supposed cases. Now, when the patient is allowed to breathe fresh air the force ceases to act with remarkable suddenness, for after a few inspirations it is gone. The result is a violent reaction—a violent rebound or whirl of the circulation terminating, in its most pronounced form, in syncope. This, and nothing else than this, I believe to be the cause of primary chloroform syncope. Let us now suppose the case in which the inhalation of the anæsthetic is continued and absorption goes on; the

internal force, acting in the same direction as the external one, *i.e.*, producing the same anæsthetic effects, gradually increases in intensity until finally the two come into equilibrium. There is now no reaction on removing the vapour, this being counteracted by the chloroform in the blood, and as this is eliminated in a slow and gradual manner, recovery also takes place slowly, and the normal state is quietly and safely regained. Short of this point of equilibrium there will be numerous points at which, if the inhalation is interrupted, oscillations of the circulation of varying degrees of intensity will occur, and by these are to be explained all the various degrees of syncope, from complete stoppage of the heart's action to a momentary pause indicated by mere transitory pallor of the countenance. All are produced, not by the absorption of chloroform, but by the elimination of the vapour from the lungs at an early stage, and a reversal of the process that ought to have gone on. Beyond the point of equilibrium—in deeper anæsthesia, the disturbing influence of the external force is neutralised ; the chloroform is now a single force acting in one direction and, with ordinary per-centages, tending to death in the most slow and gradual manner possible. Indeed, in this latter respect, I am convinced that it is by far the safest of all anæsthetics, and that this is capable of proof and demonstration.

**Facts.**—Is there any evidence in favour of this theory? Yes. In two observations, published eight years ago in Holmes' System of Surgery (3rd Ed., vol. iii., p. 618), I showed that in a cat recovering from mere stupefaction with chloroform, produced by breathing  $2\frac{1}{2}$  per cent. of the vapour in a large glass jar with a close-fitting lid for little more than a minute, the pulse rose to 200, and that this was abruptly followed by a complete pause in the heart's action lasting 60 seconds, during which there was no audible action of the organ when the stethoscope was applied to the chest. It is needless to say that there was no respiration during this pause, which was a veritable syncope. The capacity of the jar was 5000 cubic inches, so that the cat could move freely about in it, and the amount of the anæsthetic absorbed by the animal was so inconsiderable as not to affect the atmosphere to any appreciable extent. The cat was taken out when it was only so far under the influence of the agent as to lay its head flat on the bottom of the jar, and it was ascertained that for the first few seconds the heart was beating slowly and regularly before the sudden rise took place. The period of acceleration lasted about a minute before the pause occurred. On repeating the experiment the same rise in the pulse rate was observed, but the failure in the heart's action sometimes amounted only to some degree of retardation. Now, neither of these phenomena was ever observed when deep anæsthesia had

been in the first instance induced. I believe the solution of the problem lies hidden in these simple facts as in a nutshell. I regard the retardation in the action of the heart, observed by the Glasgow Chloroform Committee in a dog\*, as entirely analogous with what I have described in the cat. It is now fifteen years since I was led to conjecture, when ruminating on reported fatalities from chloroform, that some such effect was to be found, and although experiment on the rabbit gave purely negative results I was not satisfied until I made a trial on the cat, and with better success. It seemed reasonable to suppose that if the views of Simpson and Syme were correct there must be some unknown source of danger in allowing elimination of the vapour, and partial recovery to take place at certain stages of the inhalation, and, if so, that this ought to be capable of demonstration on animals.

Neither the London Chloroform Committee of 1864, nor the recent Hyderabad Commission, obtained this result, although the latter "tried hard." Dr. Lauder Brunton tells us that they even stopped short at an early stage in search of it, but I can only suppose that they were still too deep, or they must have found it. If, instead of performing various operations on animals which were merely stupefied with chloroform, they had made observations with the stethoscope or

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\* Brit. Med. Journ., June 21, 1879, p. 921.

the kymograph, they would surely have done so. That these animals did not die does not disprove this, for neither does the cat appear to die under any circumstances from this form of syncope—a fact which very much disappointed me when I first observed it. Nevertheless, I still think it reasonable to regard this as the analogue of what has so often proved fatal in the human subject, for it is the only ray of light which can be shed on the question. Recent research in some directions gives additional warrant to the belief that such may be the case. Here I would chiefly refer to that form of syncope characterised by fibrillar contraction, and spoken of by pathologists as *delirium cordis*. Professor M<sup>r</sup>William, of Aberdeen, has ascertained that this form of syncope is readily induced by galvanic and faradic shocks applied to the cardiac muscle, and that it is much more dangerous in the higher mammals than in the lower, and may cause speedy death in a dog\*. This may be of great significance in regard to chloroform syncope also. If the primary chloroform syncope cannot be induced in a rabbit at all, in which the influence of the vagus on the heart is comparatively slight, we are not in a position to deny that this form of syncope may be frequently fatal in the human subject, although the cat seems always to recover from it. And if the fatalities in the human

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\* British Med. Journ., Feby. 16th, 1889, p. 348.

subject have almost invariably been caused by this form of syncope they are to be added to the facts which go to support the above theory. I must differ from the Glasgow Committee in their statement that their dog was previously in deep anæsthesia; at all events, if my view be correct, in sufficiently deep anæsthesia the retardation of the heart would not have occurred, while, had the anæsthesia been more superficial, it would have been more pronounced. If all parties differ here, however, let this matter be re-investigated, for I take it to be the very *crux* of the problem to be solved. What is the action and reaction of certain anæsthetic vapours on the pulmonary mucous membrane? To determine this it is probable that tetrachloride of carbon, or even some substance quite insoluble in the blood, would answer better than chloroform.

Although the Hyderabad Commission could not obtain the same tracing as the Glasgow Committee had done as the direct effect of chloroform, they found they could reproduce what they regarded as identical with it by asphyxiating the animal. Hence they came to the conclusion that accidental asphyxia must have been the cause of its being obtained by the latter Committee also. But if what I have observed in the cat be the counterpart of what the Glasgow Committee found in the dog—and the cat presented a similar series of events in an even exaggerated degree—the explanation is erroneous, and I have



no doubt will be refuted by the latter on other grounds also. Possibly enough, the tracing produced by asphyxia, and that resulting from the perturbation of the circulation above described, may be the same, for during the pause in the heart's action it is probable that the arteries and capillaries are almost empty, and that the blood has accumulated in the venous system. But there was no possible cause of asphyxia in the cat, and however similar the results, the primary and sole cause was the chloroform.

**Deductions.**—It may be interesting and important to notice some of the consequences of the above principles. It will be readily granted that an agent which produces deep anæsthesia is to be regarded as a force of very considerable intensity. It may be laid down as an axiom that no force acting with great intensity on the animal system can be suddenly removed with safety. Hence, if we could conceive of the internal force, the chloroform in the blood, being removed as suddenly as the external one, the same consequence would follow, namely, syncope. We can readily imagine an agent capable of being so suddenly removed—a substance of slight solubility, but so powerful that a small quantity of it might suffice to produce deep anæsthesia. These were just the very characteristics of amylene, and furnish one reason why, in the hands of the late Dr. Snow, it

proved a treacherous and dangerous agent, the result being the very reverse of what its introducer expected. It further follows from the same principle that if the external force, instead of being removed suddenly and completely, were slowly withdrawn by allowing the percentage of vapour gradually to diminish, the tendency to syncope would be altogether obviated. Another obvious consequence is, that if the external force be not sufficiently intense, dangerous syncope will not follow. It would appear that an atmosphere of 2 or  $2\frac{1}{2}$  per cent. of chloroform vapour is required to produce an intense reaction on its removal, and, therefore, a "few whiffs" of chloroform may be given without much danger if the atmosphere does not exceed 1 or  $1\frac{1}{2}$  per cent.

The most important consequence of the theory, however, is one which deserves to be specially noticed, from its bearing on possible objections which may be urged. It is clear from the foregoing that the point of maximum danger, the point at which the worst form of syncope will occur if the external force is suddenly withdrawn, *i.e.*, if the inhalation is at once stopped, is but one of many, and that an administrator quite ignorant of it has a great many chances of escaping it. If it were not so cases of syncope would be much more common than they are, and it might evidently be objected that they ought to be more common if such an initial danger as that above indicated exists. But let us suppose that the time



required to induce deep anæsthesia is five minutes (300 seconds), and let us imagine this to be represented by a line divisible into 300 parts or segments, one of which, near the beginning, represents the point of maximum danger. It is clear that the ignorant administrator would only incur the risk of striking the fatal point once in 300 times. Again, his chance of escape would be further increased by the consideration that although he stopped short at the fatal point, he might not withdraw the vapour at once and completely, but gradually allow the strength of it to diminish, thus breaking the force of the reaction. Finally, observation in the cat shows that the reaction ensuing on withdrawing the external force lasts a minute, (in its worst form) before terminating in syncope, and during this minute the administrator has a chance of re-applying the force, and again reversing the process. In this way, then, the chance of falling into the fatal syncope might be reduced to 1 in 2000, or any given number of times. Another important consideration comes in here. While, by the hypothesis, the point of maximum danger is but one of many, points of lesser danger will be more numerous, and therefore more frequently encountered by the administrator, supposed to be ignorant of them. And hence, I believe, the reason why we so often read of pallor of the countenance being observed in the early stages of chloroform inhalation. This is nothing more nor less than a certain degree of syncope,

due to withdrawing the external force before the internal had come into equilibrium with it.

When we look at the matter a little more in detail, the manner in which the different degrees of the primary syncope are produced will become clearer. The worst form of syncope in the cat comes on after breathing  $2\frac{1}{2}$  per cent. of the vapour for little more than a minute. The reaction which ensues consists of two parts; (1), a period of acceleration of the circulation, lasting one minute, and (2) a pause in the heart's action lasting the same length of time. Now, if the theory be true, it follows that if the inhalation be continued somewhat longer, this reaction will be modified by the increased intensity of the internal force, and by the more gradual manner in which the latter ceases to act. This modification, evidently, will show itself by a lengthening of the entire duration of the reaction, by a diminution of its violence, and by a change in the relative time occupied by the periods of acceleration and retardation respectively, the latter gradually becoming shorter, until, when the two forces have come into equilibrium it disappears altogether. That is to say, by making the narcosis a little deeper the pause in the heart's action might be made to manifest itself at the end of two or three minutes, when it would be of shorter duration, and consist only of retardation or momentary failure.

If the above theory of the nature of chloroform syn-

cope be correct, it follows that we could illustrate it at will on almost every human being (except, perhaps, in parturient females, and a few others), as we can do on a cat. All that is necessary is to make any one breathe  $2\frac{1}{2}$  per cent. of the vapour for a minute or so, and suddenly stop short, when the syncope will be almost certain to show itself at the end of the next minute. This is exactly what happened in the first fatal case of chloroform syncope, in the girl at Winlaton, near Newcastle, who breathed the vapour for about a minute, and died at the end of the next minute. There can be no doubt that precisely the same thing would occur again in the same circumstances, as, indeed, has occurred in many other fatal cases. According to the theory all such cases would have been quite safe had the patients gone straight on breathing the same per-centage of vapour for five, ten, or even fifteen minutes, and this is the only thing which could have saved them. It follows that chloroform syncope can easily be avoided in every case, and I do not hesitate to record my settled conviction that there is not a man, woman, or child in the world who could not be chloroformed without the slightest risk of incurring the primary syncope. Let us now briefly review the various opinions which have been advanced as to the cause of death from the anæsthetic, that they may be seen side by side with the view suggested by the above theory.

**Theories of Death from Chloroform.**—Dr. Snow consistently maintained the opinion that death was almost invariably due to the administration of an unduly concentrated atmosphere. The suddenness of the occurrence, and the fact that the catastrophe generally happened in the early stages of the inhalation, favoured this view. Experience has amply shown, however, that it is untenable. It is emphatically negatived by numerous facts ; by the fact that a towel at 70° F. does not give off an overdose ; that the anæsthetic can be safely administered to children ; that it is safe in the hottest climates, in which it is still held in high favour ; but, above all, by the fact that fatalities have been most common in the hands of the most cautious administrators. The opinion that the deaths which have occurred in the human subject have been caused by undue prolongation of the inhalation has nothing at all to be said in its favour, so that it need hardly be discussed. Even Dr. Snow, with all his caution, admitted that this was scarcely possible. Both of the above opinions, therefore, must be dismissed ; the hecatombs of animals which have been sacrificed to prove the effects of concentrated atmospheres and of prolonged inhalation have availed nothing : the results have been as wide of the mark as one pole is from another. It is a most important fact that a cat can go on breathing a percentage of the vapour which has produced deep anæsthesia

in a given time, several times longer than the period necessary to produce the narcosis. If there is any comparison between a cat and the human subject chloroform is in this respect the safest of all anæsthetics, and from five to ten times safer than ether. I make this startling statement not rashly, but advisedly, all authorities to the contrary notwithstanding. If, then, the fatalities in the human subject have not been due to either of the above causes, we are driven to ascribe them to individual idiosyncrasies, or to some fault of the administrator of a vague kind, which cannot be precisely specified. If idiosyncrasy is to be held accountable for these catastrophes in some cases, the problem before us is to explain the nature of this idiosyncrasy, and to define it so that we can diagnose it before and not after the event—a truly difficult and impossible task. Under the new theory there will still be room allowed for the play of idiosyncrasy, but the rôle at present attributed to it merely shows the profundity of our ignorance, for individual peculiarities cannot possibly explain why people, both in India and in Scotland, can breathe with safety an amount of chloroform vapour which, in the opinion of London experts, ought to kill them ten times over. Simpson, although he soon arrived, as if by a marvellous intuition, at the proper method of administering chloroform, in which he was followed by Syme, could never satisfactorily explain the deaths which so frequently

attended an opposite method, and so failed to convince others. The suggestion that they were due to the shock of the operation failed to meet the case, as they so frequently occurred before any operation was commenced. The solitary fatal case which happened in the hands of Simpson himself was clearly one of the primary syncope; the patient moved during the first incision, vomited, and this was almost immediately succeeded by the fatal syncope. Other teachers of the Edinburgh school have been betrayed into a lamentable inconsistency, which has rendered their teaching nugatory, and which clearly shows that some leading principle has hitherto been wanting. For instance, Sir Joseph Lister laments that the anæsthetic is often given in such a way as to be almost useless, and recommends that it should be poured freely on the towel, unmeasured and unstinted. Of what avail this injunction when he elsewhere arrives at the conclusion that deaths must generally have been due to the unguarded or careless administration of an overdose of the powerful anæsthetic? Similarly with Dr. Lauder Brunton. He returns from Hyderabad and warns us that danger is from an overdose or from asphyxia. Of what use is it after this to quote with approval, as he does, (and as he has often done before) the saying of Syme that one reason of his success was that he gave "plenty of it"? The bewildered reader is impaled on the horns of a dilemma by these



contradictions. The position is an utterly inconsistent one. The advice of the Hyderabad Commission, to give the anæsthetic with excessive caution, is the very same as was recommended by Dr. Snow and the London Committee, and which Simpson summoned all his energy and vehemence to oppose. If safety is secured by giving plenty of it, there must be some danger in giving too little, and on this point the Hyderabad Commission has elicited nothing.

The theory which I have here advanced appears to furnish the only key to the solution of the enigma. The reaction which follows the sudden removal of the external force—the chloroform tourniquet acting on the pulmonary mucous membrane, explains everything. It is not the chloroform inside, but the vapour outside of the circulation which has done the fatal work on the human subject, and which has taken its departure a full minute before the fatal effect has become manifest. There is scarcely any chloroform at all in the blood when the dreaded catastrophe occurs; the little that had been absorbed having been nearly all eliminated during the reaction preceding the syncope. Hence the danger of a “few whiffs” of chloroform; hence the pallor of the countenance often seen before the second supply on the cloth reaches the patient, sufficient time having elapsed for him to have lost all that he had got. We can at once see how patients have been killed without even being anæsthe-

tised ; how the catastrophe has generally been encountered in the early stages of the administration, and in the hands of those who allow elimination to take place ; and how it has been frequent in the case of trifling operations when it has been thought that a small dose of chloroform was sufficient for the purpose. We can understand how the strongest in the prime of life have often been the victims, when the weaker have escaped, although the former would have lived twice as long as the latter had both been compelled to breathe an atmosphere of the same strength until death ensued. Indeed, I believe that the theory explains all the phenomena, and that all objections to it may be met. It may be interesting and instructive to particularise a few more cases of syncope in the human subject, in addition to the one above alluded to. A few which have been cited, or for the first time reported, since the result of the Hyderabad Commission was published, will answer the purpose.

Three very typical cases of the kind are cited by Mr. Braine in the *Lancet* for Feby. 8th of the present year, p. 316, the first being that of the girl at Newcastle already noticed. Let us take the other two. 1. Patrick C——, age not stated. March, 1848. Disease, fistula. Had taken chloroform once previously. Time of inhalation, about one minute. Quantity consumed, half a drachm. Lapse of time till death, about one minute. Patient on his side ; chloroform was administered on a handkerchief



or towel. Immediately the operation was commenced the patient became pulseless, his pulse previously being full and natural.

Whatever might be due to the shock of the operation in the above case, it is clear that according to the theory the administration was as certain to lead to syncope as if it had been a physiological experiment intended to illustrate the subject.

2. J. V.—, aged seventeen, Hôtel Dieu, Lyons, January, 1849. Amputation of finger. Chloroform was dropped upon gauze spread over the face, leaving a free passage for air. At the end of five minutes patient still felt and spoke, at the end of six minutes he became restless and still spoke. One drachm and a half of chloroform had now been given ; pulse regular and good. He now struggled violently, and within a quarter of a minute the pulse at the wrist became imperceptible. The gauze was removed, no pulse anywhere to be felt, and no cardiac sounds audible. Respiration continued, and ceased in half a minute. Means of resuscitation were employed, and respiration reappeared in two minutes' time, and then again gradually ceased. The pulse did not return.

I agree with Mr Braine that cases have gone on occurring in much the same way ever since ; indeed, it may truly be said of cases of chloroform syncope, *ex uno disce omnes*. But I dissent from his conclusion that they will continue so to do as long as chloroform is adminis-

tered. On the contrary, I say they were quite preventable. In the latter case, if an ounce and a half of the liquid had been administered in six minutes, by pouring two drachms on a towel, and renewing it every minute, the patient would have been anæsthetised with absolute safety. And similarly with the former case. But Mr Braine's citation of these cases is very pertinent and opportune. We have a right to call upon those who presume to be teachers in anæsthesia to give us their explanation of these cases. Accidents can never be prevented in the future until we know how they were caused in the past, nor until every chloroformist is convinced as to the true explanation. Were these deaths due to an overdose, or to asphyxia, or could they have been prevented by watching the respiration? No; and I assert that if my theory do not furnish the true explanation there is no other possible theory which can justify us in maintaining that chloroform is a safe agent in all cases, and the doctrine of Simpson and Syme must have been erroneous.

Numerous other fatal cases, of quite recent occurrence, appear to admit of a similar explanation, although the necessary facts are not always given in the reports. Two fatal cases in children have recently been published by two American surgeons, and these are adduced as proving that chloroform is not safe in childhood, as has hitherto been believed. In one of these, a boy of five, anæsthesia had been established not longer than one minute and a

half, and not more than a drachm of chloroform had been poured on the towel. We are told that anæsthesia was easily produced, but are not informed how long the inhalation lasted. The boy had been chloroformed on three previous occasions, when more chloroform had been used than in the fatal administration. Now, a single drachm of chloroform cannot produce deep narcosis in a boy of five, although it is quite capable of producing a reaction, terminating in syncope, in the way I have pointed out. In the other case, a girl of two, the anæsthesia is said not to have been profound, and the child cried out occasionally during the operation. The whole affair, in short, seems to have been a mere mockery of chloroform anæsthesia, as might truly be said of nearly every fatal case of administration, and it cannot be too strongly insisted on that it is not safe to give the anæsthetic in this fashion, even to children. It is hardly worth while to notice, in detail, any more of these cases, and I pass on to speak of another consequence of the above theory, which I would refer to under the designation of false anæsthesia.

**Pseudo-Anæsthesia under Chloroform.**—It follows from the views above expressed that the inhalation of chloroform may give rise to two apparently identical, but widely different conditions, which may be aptly called true and false anæsthesia. True anæsthesia results from the continued absorption of the agent, and

its accumulation to a sufficient amount in the blood, and is characterised by certain well-known symptoms. What I would call false anæsthesia is the consequence of stopping the inhalation at an early stage, and allowing elimination of the vapour to take place, when the patient may pass into a more profound state of insensibility than if he had gone on absorbing chloroform. In the worst form of this we have the aspect of death itself, and in slighter forms all the symptoms of deep anæsthesia may be present,—muscular relaxation, loss of conjunctival reflex, and stertor, all due to comparatively empty arteries and capillaries, and not to the presence of chloroform. These symptoms, therefore, no more prove that the anæsthesia is of the true sort than a temperature of 104° F. proves a case to be one of scarlet fever or of pneumonia. In one important particular the two conditions are differentiated as widely in symptomatology as they are in etiology. If false anæsthesia does not pass into death, recovery is rapid, and there is soon return of rigidity and lid-reflex, or even of perfect consciousness. In this way are explained the sudden transitions from deep insensibility to consciousness, mentioned in Dechambre's Dictionary, and elsewhere. In true anæsthesia this sudden return of the phenomena is impossible, and the distinction is of the utmost importance. How often do we read that the patient was fully anæsthetised when it is clear from the description that the

administrator knows nothing of true anæsthesia, nor of the fact that his patient could safely have breathed five or ten times more of the vapour, nay, that this was necessary to ensure safety. How often do we read that the patient took the anaesthetic badly, when it ought to have been said that he got it badly. Syncope occurring even after prolonged narcosis may still be of the primary sort, for a patient may be subjected to a number of these dangerous oscillations of the circulation—tossed as in a whirlpool, during a single administration. If a guiding principle has all along been wanting, we may be allowed to doubt the testimony of even the most experienced; those who have had the unhappy experience of being thirty or forty years in the wrong have everything to unlearn. Administrators are to be excused when eminent physiologists and pathologists, invested with a commission to inquire into the subject which gave them a sort of oracular authority, have shown their ignorance of the distinction between mere stupor and deep anæsthesia. It must be evident how bad after-effects, sickness, vomiting, faintness, and even recurrence of alarming syncope, are liable to be entailed by the faulty method of administration above indicated. Before treating of the administration of chloroform, I would make a few remarks on the alleged danger of giving an overdose.

**Danger of Overdose.**—According to the Glasgow

Committee, chloroform has a disastrous effect on the heart and respiratory centres, and a dog or rabbit will soon die under its influence, while either will live a long time if ether be used. The impression conveyed is that there is much more danger of giving an overdose of the former than of the latter. The fallacy underlying the reasoning of the Committee is so transparent as hardly to require refutation, and it seems to me they have presumed far on the supposed ignorance or want of reasoning power of the profession. It is true that chloroform is a more powerful agent than ether, a fact which was well known before the Committee commenced their investigations. But if the anæsthetic power of the former is great in proportion to its toxic influence, or power to kill, and if the dose necessary to produce a desired anæsthetic effect can be easily regulated, their contention falls to the ground. And the simple fact is that the ratio of the anæsthetic to the toxic power is greater in the case of chloroform than in that of ether, so that on the side of an overdose it is the safer agent of the two. Evidently the comparison, to be fair, should be made with per-centages of the vapour of each having the same anæsthetic effect, per-centages which might be regarded as anæsthetic equivalents. If this were done two facts could be proved. It could be shown that chloroform produces deeper anæsthesia before death ensues than ether can do, which simply means that the vital phenomena can be reduced to a smaller flame short of actual



extinction of life by the former than by the latter. Chloroform, in short, is a fine adjustment for this purpose, and ether a coarse one. Second, it could be demonstrated that when anæsthetic equivalents are used the time which elapses between a given depth of anæsthesia and death is much longer with chloroform than with ether. In this respect the former is more than five times safer than the latter. Indeed, so long is this interval that the question whether the heart or respiration fails first I regard as of no practical importance whatever. Never had anything less foundation in fact than the prevalent idea that a patient in deep chloroform narcosis is tottering on the verge of a precipice, where the slightest touch, or the slightest additional whiff, will push him over. I incline to the opinion that he is rather safer than the average individual, exposed to the ordinary casualties of life. If it be said that there is still the danger of an unduly concentrated atmosphere of chloroform to consider, the answer is that this danger is purely visionary. As will be shown a little further on it is impossible to get an overdose from a towel at 70° F. however freely the liquid may be poured on it. The cautious Dr. Snow restricted the limit of safety to 5 per cent., and this is much beyond what can be got from a towel at the temperature mentioned. Let any one attempt to apply his nose to the mouth of a jar charged with 5 per cent. of chloroform vapour and he will be readily convinced that an atmosphere of this strength has never been

administered to the human subject. I hold the conclusions of the Glasgow Committee on this important point, therefore, to be entirely erroneous, and in the highest degree pernicious in their tendency, which naturally is to deter any administrator from deep chloroform narcosis—the only safe state of anæsthesia under this agent.

### **Fall of Blood-Pressure under Chloroform.—**

Before the London Committee of 1864 commenced their series of carefully conducted experiments, clinical experience had amply shown that chloroform was a dangerous agent. Finding, therefore, that when administered to animals it promptly lowered the pressure in the vessels, they naturally inferred that this afforded one indication of its dangerous character, and this view has been almost universally accepted. There are grounds for calling even this in question, however, and I gather from various statements of Dr. Lauder Brunton that he does not unhesitatingly accept it, but seems rather inclined to an opposite opinion. I hail this with satisfaction, as I have long believed the view of that Committee to be unfounded. Indeed, if the statement made in the preceding paragraph be a fact, that life is more slowly extinguished under chloroform than under an anæsthetic equivalent of ether, it must be so. This fact, indeed, is partly due to the greater lowering of the pressure under chloroform, and partly to the slighter solubility of this agent as compared with ether,



circumstances which combine to permit of its passage into the blood in only a very gradual manner, especially beyond the stage of surgical anæsthesia. But whatever the reason of this great prolongation of life under chloroform, the practical value of the fact is the same. I believe that the above are not the only arguments which could be adduced in favour of the opinion that the greater power of chloroform to lower the blood-pressure is at once an indication of its superiority to ether, and of its greater safety on the side of an overdose.

**The Chloroform Atmosphere and how to keep it up.**—In order to reduce these principles to practice, we must know how to keep up a sufficient atmosphere of chloroform. The amount of vapour given off from a towel depends on the temperature and dryness of the air. Sir Joseph Lister performed an experiment to show that there is no danger of getting an overdose of vapour from a towel, even at 70° F.\* He found, by weighing a towel on which  $1\frac{1}{2}$  drachms of chloroform had been poured, and circumstanced as nearly as possible as in actual inhalation, and estimating the loss of weight during one minute's evaporation, that the atmosphere given off did not exceed  $4\frac{1}{2}$  per cent., supposing the patient to inspire the average amount of air necessary for a healthy

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\* Holmes' System of Surgery, 3rd Ed., Vol. iii., Article—*Anæsthesia*

adult. In repeating this experiment, which I have done carefully several times, I was struck with the fact that I never got a stronger chloroform atmosphere than  $3\frac{1}{2}$  per cent., and the result was the same if half-an-ounce of the liquid was poured on the cloth. In the course of the experiment another point worth noticing was ascertained. After two minutes evaporation, although the towel smelt strongly of chloroform, very little more vapour was given off—so little that a patient breathing the atmosphere must have been eliminating, instead of absorbing, the anæsthetic. This is of the utmost importance in connection with the objectionable practice of many who do not renew the dose of liquid until the smell of the anæsthetic has left the cloth, and who go on frequently smelling the latter instead of making the patient do so, a practice truly ludicrous were it not dangerous. To keep up a proper atmosphere of the vapour, therefore, requires an expenditure of two drachms of the liquid every minute (or at a little longer interval), and it is well also to have a temperature of  $65^{\circ}$  F.

I witnessed some ridiculous scenes of chloroform administration on my first coming to this city. I cannot forget the terror I have seen depicted on the countenance of some old and experienced practitioners when one insisted on renewing the dose on the towel. And I well remember one case in which pallor of the countenance and disagreeable after-effects, lasting for several months,

according to the patient's account, resulted from the practice of the administrator smelling the towel before renewing the dose. It seems that he had always seen this done in the Royal Infirmary, and I could hardly prevail on him to desist, or to allow the dose to be renewed, and the result was very unsatisfactory (and, as I am now thoroughly convinced, dangerous), anæsthesia, at the end of fifteen or twenty minutes. He was afterwards persuaded, however, to adopt a different method, and expressed great surprise at the difference in the result. Such scenes are still common, and I believe that one half of the profession have hardly any conception of the manner in which chloroform is administered by the other.

**Administration of Chloroform.**—It would appear from the foregoing that chloroform is an even more formidable weapon than had previously been supposed; that it is, in fact, a two-edged sword with which the unskilful should not meddle, and in particular a very dangerous agent for self-inhalation. The initial danger at the outset has to be added to the further risk of giving an overdose in the deeper stages of narcosis, so that the chloroformist must steer between Scylla and Charybdis, and his motto must be : *in medio tutissimus ibis*. Hitherto he has only been warned against being wrecked on the old rock Scylla, which has been well enough known and recognised since the first day the anæsthetic came into

use. The Hyderabad Commission has but added another to these admonitions to avoid Scylla, the result being of course to drive the mariner with all the more certainty into the whirlpool on the other side of the channel. It is fortunate that the conclusions of the Commission, as is clear from various letters which have appeared in the medical journals, are not generally regarded as satisfactory. The results of clinical experience, and the opposite doctrines of different schools as to the proper method of administration, are not explained by the work of the Commission. Already they have been confronted with these clinical results—with cases which have proved fatal in two minutes or so, in which no overdose had been administered, and in which there is not the shadow of a reason for saying that the primary syncope could have been avoided by watching the respiration. The Commission has made matters worse than before, for they have declared that death from chloroform is certainly preventable without having in the least shown how this consummation is to be attained. The result will not be to induce those who are already too much afraid of chloroform to give it freely, like Syme or Surgeon-Major Lawrie himself, but the very reverse, and with a probable increase in the mortality. It was not by watching the respiration, but by giving “plenty of it,” that Syme achieved his results. By administering a continuous atmosphere he was clear

of Charybdis in a few minutes, when nobody was watching anything, and it was only in deep anæsthesia that he sometimes found it necessary to watch the respiration. The admonition to watch the latter applies correctly to Scylla only, and not to Charybdis ; those who run the risk of falling into the latter have much need to fear a sudden stoppage of the pulse.

The Glasgow Committee stumbled on Charybdis, for they had not previously suspected its existence. They were far from appreciating its importance, however, and, in fact, failed even to recognise it, for they were careful to state that the dog did not receive an overdose, and evidently had no suspicion of the fact that the retardation of the heart observed by them was due to the animal having got an underdose. They leave us to infer that this danger may be encountered anywhere in chloroform narcosis, and that we are never to know when and where. If this were so, then indeed the case would be finally closed against chloroform. But it is not so ; Charybdis occupies a definite position ; its latitude and longitude may be exactly defined, and the mariner provided with a chart which will enable him to steer clear of it with ease and safety. My observations on the cat, and the experience of the boldest administrators, who have met with the greatest success, prove conclusively that this is the case. There is plenty of room in mid-channel, and the dangers on either side

have but to be known to be easily avoided. Hitherto, the experience of the chloroformist might be truly expressed (with a transposition of two words) by the line :

*Incidit in Scyllam qui vult vitare Charybdim.*

But he need neither strike against the one nor go down in the other.

The above principles point to a single plain rule for the administration of chloroform. This is to keep up a continuous atmosphere of the vapour until deep anæsthesia is induced, and not to stop short of this point for any reason whatever, and no matter what the operation. The chloroform tourniquet having been once applied to the pulmonary circulation is to be kept there until the internal force comes into equilibrium with it. I here record my strong conviction that whenever this is done in all cases chloroform syncope will become a thing of the past. It may be objected : Are we not to stop for a few seconds when the patient says he is being choked, or when he holds his breath, or coughs, or struggles violently ? The answer to each and all of these questions is an emphatic NO. By bringing the towel gradually near during the first few seconds the feeling of choking may be nearly prevented, but if the patient still complains of it the administrator should say, never mind, all the better ; and go on. When the patient holds his breath for a considerable time, as he often does from the diminution of the *besoin de respirer*, there is no



harm in waiting till respiration spontaneously returns, but as this causes loss of time it is better to tell him to go on breathing so long as he can hear and respond. With respect to struggling and excitement it will be found that they are reduced to a minimum by the continuous method of administration here recommended. Dangerous struggling may be caused by the interrupted method of inhalation. What of alarming pallor of the countenance? This, the slightest form of the primary syncope, will never be seen; on the contrary, it will be found, as I have constantly observed, that there is a distinct heightening of the complexion in children and people with transparent skins. So with regard to vomiting, which only occurs during elimination, and not while absorption is going on. It ought to be known, too, that salivation is an indication that the atmosphere being breathed is weak, for it will not occur with one of 3 or 4 per cent. I take this salivation to be another proof of the local action of the vapour, and there can be no doubt that it exerts a powerful influence on the whole respiratory tract.

It seems to me that some details of Surgeon-Major Lawrie's clinical experience would be valuable, perhaps more so than all the results of the Hyderabad Commission. He says that he has never had a case of syncope. Has he ever seen any dangerous symptom within the first five minutes, or before the stage of deep anæsthesia was reached? How long does he generally take to induce satis-



factory narcosis, and what is the amount of liquid used in doing so? How much is employed in a given time in the course of long operations? What is the dose poured on the towel, and how often is it renewed, as a rule? What is the temperature of the air in the hottest weather, and does he then find it necessary to be very careful to guard against an overdose? Answers to these questions by one who has had 10,000 successful administrations would be valuable for comparison with those reports of fatal cases with which we are all too familiar.

So far from there being any danger of getting an unduly concentrated atmosphere of chloroform vapour in our climate, the difficulty is all the other way, to get it strong enough. Since performing the experiments with the towel already mentioned I constantly take care, when giving chloroform, to have the apartment heated to 65° or 70° F., and to have two towels thoroughly warmed and dried at the fire. Two drachms of the liquid is poured on the cloth, which is then held close to the nose and mouth, and this dose is repeated in little more than a minute, and always in less than two minutes. Each successive dose is poured on a different part of the towel, not cooled by previous evaporation, which is then reversed so suddenly that the patient never gets a breath of air unimpregnated with the proper per-centage of the anæsthetic. By this procedure there is no danger of dropping any of the liquid on the lips or nostrils, which would certainly

be objectionable, and probably dangerous. I often open up the towel so as to have only one or two layers over the patient's face, the object being to hasten evaporation as far as possible, and for this purpose also the second warm towel is brought into requisition if necessary. Throughout the entire process my sole care is to see that the atmosphere is kept up ; I watch the bottle and the towel and pay nothing but an incidental attention to the pulse or respiration, satisfied that nothing can go wrong with these short of the deepest surgical anæsthesia. Let the atmosphere be watched, and those deputed to watch the countenance, pulse, or respiration, will find their occupation gone. It is truly ludicrous to picture half-a-dozen skilled attendants round an operating table, watching for an event which should never happen. The above method may be wasteful, for it requires several ounces of chloroform to induce anæsthesia, but it is not hasty or reckless, as might be thought, for true anæsthesia is not produced by it in less than five minutes, even in a child of ten, and the atmosphere is probably never more than 3 per cent. Drops of chloroform may produce false anæsthesia and even death, but according to my experience, it is impossible that true and satisfactory narcosis can be induced by these small quantities. If the above views are correct they show that the teaching of Syme and Simpson rested on a truly scientific foundation, and that the practice of many must be

radically altered. The interrupted method of administration must be for ever abandoned ; the administrator must recognise that there is no danger in the early stages but in going backward, and it will soon be found that there is no more marvellous instrument of precision, certainty, and safety in the whole range of the *materia medica* than chloroform.

## APPENDIX.

Since these sheets were put into the hands of the printer there has appeared in the *British Medical Journal*, of June 14th, an article by the Glasgow Committee, entitled "Remarks on the Report of the Second Hyderabad Chloroform Commission," and this was followed next week by a rejoinder in the *Lancet* from Surgeon-Major Lawrie, along with some further details of the work of the Commission.—(*Lancet*, June 21, p. 1369). Before commenting on these I would, on further consideration, draw attention to some statements made by the Commission in their first report, published in the *Lancet*, of January 18th, 1890, p. 149, with respect to blood-pressure. These seem to me of special importance in connection with one of their experiments, to be immediately noticed. If, in the preceding pages, I have overlooked this experiment, it is largely to be accounted for by the fact that it is only referred to in a very general way, and what I regard as necessary details of fact are altogether wanting. I still labour under the same disadvantage, but shall try to make the most of it in the circumstances. The Commission found that the administration of chloroform caused a continuous fall of blood-pressure, and that after the inhalation was discontinued there was still an after-fall for some time, the rate of which depended altogether on the rapidity

of the fall while the chloroform was being inhaled. They have expressed the belief that this after-fall is probably due to absorption of a portion of the residue of chloroform in the air-passages after the stoppage of the inhalation. The occurrence of this after-fall corresponds with what was long ago pointed out by Dr Snow and M. Sédillot, viz., that the effects of chloroform increased after the inhalation was left off, owing to the absorption into the system of the vapour which was contained in the lungs at the time when the inhalation was discontinued.\* Dr. Snow, however, makes the further important remark that this accumulation or increase of the effects of chloroform lasts for about twenty seconds only. In the beginning of his work he points out, from observation of the habits of eastern nations, who inhale when they smoke, that the lungs become emptied of their contents by three rather full expirations and inspirations.† He describes the process as witnessed by himself, “When this gentleman (a smoker) took the cigar from his mouth to speak, the smoke could be seen issuing thickly with each word till there was a momentary pause as he took a fresh inspiration, then the smoke could be seen issuing with each word as before, only not so thick, and after another inspiration, the smoke could be still perceived in the expired air, but in a very diluted state; but after a third inspiration, it could no longer

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\* Snow on *Anæsthetics*, p. 91.† *loc. cit.*, pp. 12 and 25.

be seen till he had resumed the cigar." I may here remark that it was partly on this observation of Dr. Snow that I based my statement with respect to the rapidity with which the vapour ceases to act in the lungs when the inhalation is stopped. I readily grant that an after-fall of blood-pressure may be explained, at all events partly, by this absorption from the residue of vapour in the lungs, but I would say that such a fall must be continuous and limited to the time mentioned by Dr. Snow, viz., twenty seconds, or not much more. Passing over the fall caused by the animal holding its breath, or by stopping its mouth and nose and producing temporary asphyxia, the Commission refer to another severe and prolonged after-fall which may occur in an animal in which the chloroform is pushed without sufficient dilution with air, causing struggling and alternate holding of the breath, until respiration ceases. This also has no bearing on the question to which I wish to refer. But the Commission mention still another fall of pressure under apparently different circumstances, and it is this to which I desire specially to direct attention.

Speaking of artificial respiration they say, \* "the success of artificial respiration in restoring the blood-pressure is in some cases very remarkable; *vide* especially experiment 40, in which the heart had apparently ceased

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\* *Lancet*, January 18th, 1890, p. 156.



beating, and the dog was believed by every one present to be dead, and yet recovered with artificial respiration. The success in this instance is due to the fact that chloroform had only been administered for a few seconds, and that the depression was the result, not of continuous chloroform administration until respiration ceased, but of a long and severe after-fall."

In the succeeding paragraph they go on to comment, in what appears to me an extraordinary manner, on this experiment. "It corresponds to those cases which are so often reported, in which dangerous failure of the heart is said to have occurred some minutes after the administration of chloroform had been discontinued, and which are sometimes restored, and sometimes not, by artificial respiration. There is nothing at all sudden about the failure of the heart in these cases, but the attention of the chloroformist, which had been wandering, is suddenly called to the fact that the patient is apparently dead."

Of the 600 experiments performed by the Commission one may be more valuable than all the rest, and I certainly take more interest in this one than in the remaining 599. I should like to know every detail of this interesting observation. It has always seemed to me that one of the most important statements made by the London Committee was that the heart might sometimes cease before the respiration, even when very dilute vapour was being inhaled, and that they should have given every particular



about the case in which this was observed, but as far as I could find they did not do so. And so I think the Commission should have told us everything about the above case, and should have repeated the experiment under the same conditions. The most important facts communicated are that the respiration had not ceased when the inhalation was discontinued, and that there was a long and severe after-fall. But we want to know much more. Passing over the per-centage of vapour breathed by the animal, which the Commission have not taken the trouble to estimate in any case, an omission which very much detracts from the value of their experiments, I would ask, How many seconds was the inhalation continued? Was the animal supposed to be in deep anæsthesia when the administration was stopped? Was the after-fall of pressure continuous from the first? How long did it last?

As far as the description goes it seems to warrant me in connecting the case with that of the cat already mentioned. The dog had inhaled the vapour only for a short time, and a state of apparent death had ensued sometime after it had been allowed to breathe fresh air. If so, the Commission have altogether failed to explain it, and it is hardly possible to conceive of anything more illogical than their reasoning on the subject. It would appear that the agent that may produce this alarming effect, even after its use has been discontinued for some

time, is still quite a safe one. It is as clear as day that if there is not some principle of administration which renders us quite free from this subsequent danger, chloroform is for ever condemned. According to the Committee it (the above experiment) corresponds to those cases in the human subject in which dangerous failure of the heart is said to have occurred some minutes after the administration of chloroform had been discontinued, and concerning which they go on to make the further sapient remark that there is nothing at all sudden about the failure of the heart in these cases, but the attention of the chloroformist, which had been wandering, is suddenly called to the fact that the patient is apparently dead. If this case of the Commission really corresponds, as it appears to do, to cases of syncope in the human subject, it illustrates everything for which I contend. The heart failure in both is produced by stopping the administration too soon. The case stands in direct contradiction to a previous statement of the Commission to the effect that if the administration of the chloroform is stopped at an early stage, the pressure very soon begins to rise again, and gradually becomes normal. A prolonged after-fall could not have been caused by absorption of some of the residue of vapour in the lungs, neither in the case of an animal which was still breathing when the inhalation was stopped could such a lengthened fall have been a continuous one. The fall of pressure observed by the Glasgow Committee

was not a continuous one, lasting from the time when the chloroform was left off, for when the connection with the kymograph was first made (two minutes after the stoppage of the inhalation) the pressure was nearly normal. Would the Hyderabad dog have died without artificial respiration? If so, it would have still further corresponded with cases in the human subject, for it would have been a death from the primary syncope, but it would have differed from the cat, which recovers from the same apparent death with the same certainty without artificial respiration as with it. The Commission tell us nothing about any previous failure of the respiration in the above case, nor how the stoppage of the heart could have been prevented by watching it. Neither the heart nor the respiration, as far as I have observed, can be influenced by watching them, and if they cannot be left to look after themselves some minutes after the chloroform has been withdrawn the agent is an excessively dangerous one, and its use should be abandoned. As well tell a captain that safety is to be secured by watching the prow of the vessel. This, it is evident, can only apprise him of the earliest occurrence of disaster which may be irretrievable. The advice is inadequate ; he must know the external danger—the situation of the sunken rock or the treacherous whirlpool. And the danger to the chloroformist lies in the chloroform atmosphere. Failure of the heart after the chloroform is withdrawn can only be certainly prevented

by making the anæsthesia sufficiently deep in the first instance.

The Glasgow Committee, in the article above referred to, point out that the tracings obtained by them in dogs under chloroform are not identical with those caused by asphyxia. As I have already remarked, whether this be so or not, it can be distinctly proved that they are directly due to the chloroform. If direct stimulation of the vagus may produce the same tracing as asphyxia, so may some other influence bring about a state of the circulation which will give the same result. But the most important statement made by the Committee is that they agree with the Commission in explaining the retardation of the heart, observed by them two minutes after the chloroform had been withdrawn, by the supposition that it was probably due to absorption of a portion of the residue of chloroform in the air-passages after the stoppage of the inhalation. This proves how far the Committee are from seeing what I apprehend to be the true explanation of the case. And the hypothesis can be easily disproved. Dr. Snow was right in limiting any effect of this kind to about twenty seconds. The fall observed by the Committee was not continuous, and if it were caused as they suggest it would be quite impossible to give chloroform with safety. But one fact negatives their argument completely. It is that when the anæsthesia is made sufficiently deep in a cat the pause in the heart, which I have above described, does not occur

during recovery, and neither would the sudden fall of pressure observed by the Committee take place in the same circumstances. Both are due to the reaction ensuing in the lungs after the vapour is withdrawn, and this explains the difficulty, and finally settles the matter.

In the last report of the Hyderabad Commission\* they have not yet given us the details of experiment 40. In the experiments of which details are furnished there is nothing whatever bearing on the point. In this report Surgeon-Major Lawrie says that the fall of blood-pressure produced by chloroform is in no sense a danger . . . unless it is excessive—that is to say unless an overdose of chloroform is inhaled. Then we are to suppose that the dog in experiment 40 had really inhaled an overdose. How is this supposition to be reconciled with the fact that it had only breathed the vapour for a few seconds? It lies with the Commission to harmonise these apparent contradictions.

While willing to admit that asphyxia may produce all the results attributed to it by the Commission in chloroform narcosis, I nevertheless express my conviction that it has had nothing to do with the fatalities which have occurred in the human subject, and that it is no source of danger in the ordinary administration of the anæsthetic. The conclusions of the London Committee on this point were, in my opinion, much more accurate. Surgeon-Major

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\* *Lancet*, June 21, 1890, p. 1369.

Lawrie's description of the ignorant and timid administrator asphyxiating the patient is drawn entirely from his own imagination. He does the very reverse: he gives the patient air when he ought to have continued the chloroform, and syncope is often the consequence. One would suppose that Surgeon-Major Lawrie was quite ignorant of the clinical records of chloroform syncope. The fatal catastrophe has often occurred in the hands of the most experienced and cautious, the excess of caution, in my view, affording the real explanation of its occurrence.

It might be supposed that after administering chloroform "in every conceivable way," as the Commission claim to have done, all the facts in connection with the subject must have been elicited. But this is far from being the case, and the Commission do not even seem to be cognisant of many facts which have been accumulated by their predecessors. Not a single member of the Commission, or of the Glasgow Committee, could answer a number of simple questions which could be put to them on anæsthesia. The most interesting and instructive fact in connection with the Commission is that it has been a stupendous failure.









Chloroform Syncope

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